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Are criminals strategic? Offender responses to drug sentencing cutoffs☆

Louis-Pierre Lepage

Department of Economics, University of Michigan, Ann Arbor, 48109, US

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ABSTRACT

In many US states, punishment severity for drug offenses increases discontinuously at established quantity limits. Theory predicts that these laws create bunching of offenders at these limits and dominated regions beyond, where it ex-ante appears irrational to locate. These policies provide a novel setting to test for deterrence along a different margin that typically considered in the literature and generate new evidence on the extent to which offenders respond to incentives. Using arrest reports from 28 states and leveraging variation from quantity cutoff changes through time, I find evidence of strategic bunching by offenders in response to cutoff policies.

A large literature on the economics of crime studies how offenders respond to changes in incentives to engage in criminal activity, with implications for key policy decisions in areas like policing, sentencing, and incarceration. The Becker (1968) model remains the most influential framework in the literature. The premise of the model is that offenders are rational and choose whether to engage in criminal activity by weighing the potential benefits of a crime against its costs. This notion is more or less explicitly at the center of most work in the literature, yet the extent to which it accurately captures decision-making remains an open question, especially in the context of deterrence and sentencing severity. As Chalfin and McCrary (2017) conclude from their review, the relationship between crime and the severity of punishment is one for which there is comparatively little compelling evidence.

In this paper, I analyze arrest reports of drug seizures to test whether offenders are strategic and choose the optimal amount of contraband to carry or transact with. The intuition is simple and parallels the literature on tax notches.[1] For drug crimes, offense severity is often discontinuously affected by the quantity of contraband seized during the arrest. This gives rise to quantity cutoffs where punishment increases discontinuously for any amount exceeding the limit. Accordingly, the expected value of the crime falls discontinuously at the cutoff. In theory, this should lead offenders to bunch at these limits if the policies create dominated regions in which it is ex-ante irrational, in the Becker sense, for offenders to locate.

This leads to a simple prediction. There should be excess mass at cutoffs and missing mass beyond. In practice, testing this prediction is complicated by the fact that bunching arises naturally through market transactions which often involve round, conventional quantities. Since cutoffs are also often set at these common quantities, this "natural" bunching from regular market activity is a priori indistinguishable from strategic bunching in response to cutoff policies. I overcome this challenge by leveraging plausibly exogenous variation from legislative changes in cutoff policies across states and time to isolate strategic responses.

Using over 3.5 million arrest reports in 28 states from the National Incident-Based Reporting System (NIBRS) between 1998 and 2014 and using variation from 36 changes in cutoffs through time, I uncover clear evidence of strategic reactions to cutoff policies. Graphical evidence as well as results from a difference-in-differences (DID) strategy similar to difference-in-bunching designs (Kleven, 2016) (Tuttle, 2019) and synthetic control group approaches show that offenders are more likely to carry a given quantity after it becomes a cutoff limit. These effects are generally statistically significant, substantial, and robust to several measurement or law-enforcement manipulation considerations as well as changes in specifications. They correspond to an approximate 36% increase in the fraction of offenders who locate within one gram of the cutoff for marijuana and a 16% increase for crack, cocaine and heroin. This increased mass comes from above the new cutoffs as well as from below, indicating that offenders also adjust to cutoffs by shifting up to

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E-mail address: lepagelp@umich.edu

[1] See Kleven and Waseem (2013) for an example of tax notches, which arise when there are discontinuities in the choice set of an individual due to discrete level changes in tax liability. Notches differ from kinks as considered in Saez (2010) and Chetty et al. (2011), which are discontinuities in the slope of a choice set due to changes in marginal tax rates at certain cutoffs.

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higher quantities. Looking instead at cutoffs being removed, also consistent with theory, evidence suggests that offenders are less likely to carry a given quantity after it stops being a cutoff limit.

The design is a simple application of the Becker framework, with implications for the broader literature. Unlike most existing work, I do not investigate whether punishment severity influences an individual's decision to engage in crime (the extensive margin). Rather, I investigate whether it influences *how* the crime is committed and provide direct evidence that punishment considerations influence the decision-making of offenders. The reaction of offenders in this simple context can be seen as a minimal condition for general deterrence.[2] It would seem difficult to argue that changing punishment severity could lead to substantial general deterrence if clear discontinuities in punishment do not influence behavior along the margin considered here, let alone the primary decision to engage in criminal activity. Moreover, my results imply a detailed knowledge of drug laws and a careful consideration of the risks of apprehension by a subset of offenders in the market, contributing direct evidence to these mainly survey-based strands of the literature.[3]

The setting is particularly well-suited to study whether offenders respond to economic incentives. Chalfin and McCrary (2017) document two key identification challenge often arising in studies considering sentencing policy changes across states. First, since the supply of crime at any point in time depends on a wide range of time-varying factors and interventions, isolating the effect of sentencing policies from these other factors is difficult. Second, there is a mechanical relationship between incarceration and the supply of crime through incapacitation, which can lead to simultaneity bias. This paper avoids these issues by focusing on changes in the likelihood of offenders being arrested with specific quantities of contraband, which leaves less room for confounders and allows me to rule out that incapacitation drives the results.[4]

The rest of the paper is organized as follows. Section 1 outlines the theoretical framework. Section 2 discusses the data and cutoff policies which give rise to discontinuities in sentencing severity. Section 3 presents the empirical strategy used in the analysis. Section 4 presents the findings, and Section 5 concludes.

1. Theoretical framework

The Becker model posits that offenders choose whether to engage in criminal activity by weighing the potential benefits of a crime against its costs.[5] For drug crimes, the benefits can be seen as the utility of consuming the drugs or the revenue from their sale. The expected cost, net of financial considerations, is an increasing function of punishment severity. For simplicity, the benefits can be represented as a continuous increasing function of the quantity, while cutoff policies introduce discontinuities in expected costs. These discontinuities give rise to dominated regions above cutoffs, in which the net expected benefit of a crime is strictly lower than at the cutoff.

Fig. 1 illustrates the decision of an offender optimizing the amount of contraband they carry. A cutoff at Q_1 creates a discontinuous increase

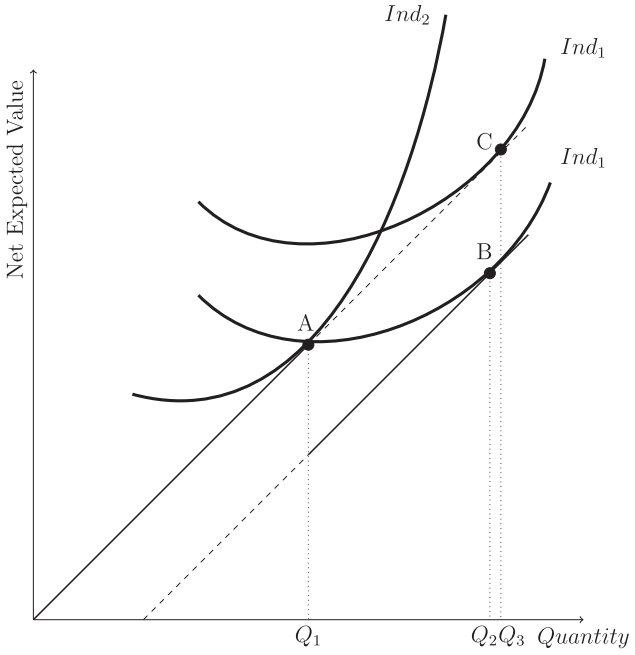

Fig. 1. Quantity cutoff (Q_1) and the expected value of crime.

in punishment and a discontinuous decrease in the expected value of a crime for quantities above Q_1. Individual 2 is the individual who would have carried the lowest quantity before the introduction of the cutoff (Q_1) and who now chooses to locate at the cutoff. Individual 1 is the individual who would have carried the highest quantity before the cutoff was introduced and who now chooses to locate at the cutoff. This individual chooses quantity Q_3 in the absence of the cutoff, but is indifferent between Q_1 and Q_2, which lies strictly below Q_3, in the presence of the cutoff. Individuals between 1 and 2 should locate at the cutoff point and none should locate in the region between Q_1 and Q_2. The size of this dominated region represents the maximum amount of contraband that an individual is willing to give up to avoid additional punishment.

One consideration is whether offenders necessarily give up excess amounts above the cutoff or partly re-organize trade into smaller transactions. Offenders may adjust the volume of transaction, the quantity traded per transaction, and the total quantity traded. For example, a cutoff at 5 g may incentivize an offender wishing to buy 10 g to conduct two transactions of 5 g rather than one larger transaction. Since increasing the frequency of transaction may increase the probability of arrest, offenders face a trade-off, suggesting that they may partially adjust the volume of transaction for larger excess quantities and give up small excess quantities. Regardless of these adjustments, bunching at cutoffs provides evidence of strategic offender adjustments to these policies, but implications regarding deterrence of drug trade and consumption depend on these considerations. See Appendix A.5 for a more detailed discussion and test of this auxiliary hypothesis.

In practice, the size and existence of dominated regions are also affected by market features and optimization frictions which could lead individuals to carry amounts between Q_1 and Q_2. For example, aggravating factors such as criminal history can sometimes lead to higher charges regardless of quantity. Similarly, potential discounts or changes in substance purity for larger transactions could decrease incentives of offenders to bunch at certain cutoffs. It is therefore expected that the empirical density distribution around a cutoff exhibit partial bunching with additional mass to the left of the cutoff and missing mass beyond.

[2] Recent work such as Lee and McCrary (2017) as well as Mueller-Smith and Schnepel (2019) show little evidence of extensive-margin responsiveness, while another body of evidence in Abrams (2012); Bell et al. (2014); Drago et al. (2009); Helland and Tabarrok (2007) and Hansen (2015) does find evidence of deterrence. In contrast, the literature on deterrence and policing typically finds substantial effects. See Chalfin and McCrary (2018) and Mello (2019) for two recent examples.

[3] See Apel (2013) and Nagin (2013) for an overview of these topics.

[4] Incapacitation could explain a relative increase of offenders at the cutoff versus above if offenders who fail to bunch are incarcerated longer, but cannot explain a net increase in mass at the cutoff or additional mass coming from below new cutoffs.

[5] While I focus on costs, Draca and Machin (2015) review work on benefits and conclude that offenders respond to changes in the returns to crime. Draca et al. (2018) also provides evidence that criminals respond to price fluctuations in the case of property crimes.

2. Data and cutoff policies

I use the property segment of the NIBRS, part of the Federal Bureau of Investigation's Uniform Crime Reporting program data, which contains information on drug reports submitted by law enforcement agencies.[6] Most importantly, the incident reports include the type and quantity of drugs seized (up to three types) for each arrest, as well as the arresting agency, date, and state of the arrest.[7] Procedure concerning drug arrests differs across agencies, but typically dictates that suspicious substances should be field tested for identification and, conditional on being identified as an illegal drug, weighted before packaging and submitting as evidence. This information is included in reports filed by the officer(s) in charge of the arrest, which provide the foundation of the NIBRS data. I consider reports from 28 states from 1998 to 2014, and a summary of the state-year coverage is presented in Appendix A.1.[8]

One concern is whether law enforcement could misreport or manipulate the quantity of contraband seized to affect punishment severity. Goncalves and Mello (2018) study discontinuities in punishment for speeding and find evidence that officers routinely reduce the speed written on tickets to decrease fines. Tuttle (2019) investigates whether there is abnormal bunching in the distribution of crack-cocaine for federal sentencing cases at the arrest and prosecution stages. They study a 2010 increase in the 10-year mandatory minimum threshold for crack-cocaine from 50g to 280g. They do not find evidence of strategic bunching by offenders or law enforcement at the arrest stage, but do find increased bunching above the new threshold later in the sentencing process, which is disproportionately large for black and Hispanic offenders, and which they attribute to discriminatory prosecutorial discretion.[9] The contexts and pool of offenders considered in these papers differ importantly from mine, and I present evidence that such type of manipulation cannot explain the patterns of adjustment I document in the data.

Compared to Goncalves and Mello (2018), the procedure and offense severity differ markedly.[10] Tuttle (2019) considers a more similar context, but focuses on a large 280g cutoff worth several thousands of dollars and often involving joint operations by law enforcement with federal agencies. In contrast, I concentrate on strategic offender responses at the arrest stage, focusing on much smaller routine police seizures (50 times smaller on average). The fact that they find no evidence of police manipulation at the arrest stage supports the validity of my analysis, the type of manipulation they document at the prosecution stage would go in the opposite direction than bunching as I document, and differences in context can help explain why they do not find evidence of strategic offender responses at the arrest stage.[11] Further, such police manipulation does not explain why some additional mass at new cutoffs comes from below these cutoffs as I document, and dominated regions appear too large to be consistent with leniency through deliberate manipulation.[12] Lastly, in the spirit of the discrimination test of policing leniency from Goncalves and Mello (2018), I test whether bunching behavior varies by demographic characteristics in Appendix A.4. I find that it doesn't for hard drugs, but that African-Americans and men are less likely to bunch at cutoffs for marijuana. While this is consistent with officers potentially showing less leniency to black offenders and men for marijuana crimes, and manipulation is presumably more likely for these crimes given their lower severity, it naturally does not constitute definitive evidence of either manipulation or discrimination.

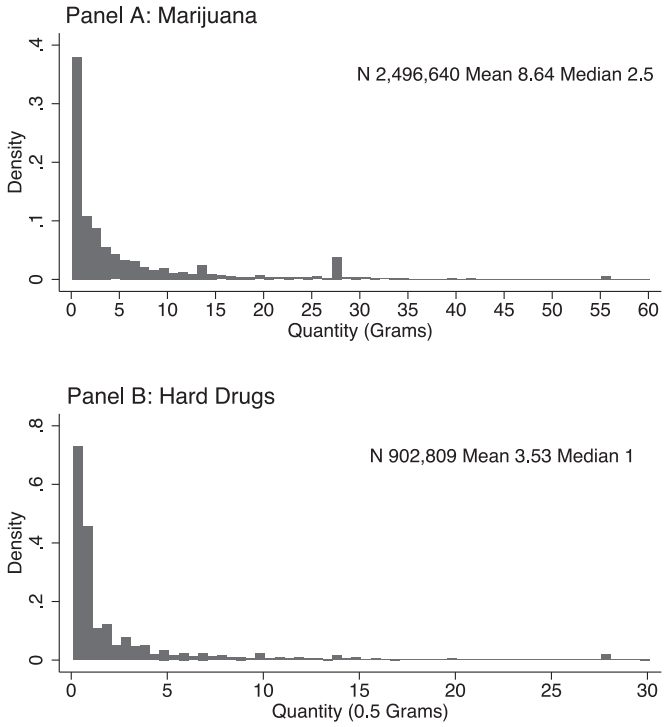

Fig. 2. Distribution of drug seizures. See Appendix A.1 for states and years included in the analysis. Source: NIBRS 1992–2014.

I focus on four types of drugs: marijuana, cocaine base (crack), cocaine and heroin which account for nearly 90% of reports. I pool changes in cutoffs for crack, cocaine and heroin together due to their relative similarity in legislation, severity, value and quantity traded. Invalid or incomplete entries were removed as well as observations from a few states which reported rounded quantities (around 7% of reports in total). Since there are relatively few reports and cutoffs for large amounts, the analysis is restricted to cutoffs no more than two ounces (56 g) for marijuana and 10 g (30 for Ohio) for other drug types. The resulting sample consists of 3,560,334 contraband seizures whose distribution is presented in Fig. 2 and highlights that the vast majority of seizures involve smaller quantities.

[6] The NIBRS data is publicly available from the Inter-university Consortium for Political and Social Research (ICPSR) at the University of Michigan and can be obtained at https://www.icpsr.umich.edu/icpsrweb/NACJD/NIBRS/.

[7] Quantities seized refer to total amounts found in an offender's possession, potentially including quantities obtained from vehicles and other searches.

[8] Although law enforcement agencies are encouraged to participate in the UCR program and increasingly more do so, participation remains voluntary and can vary from year to year. I make no claim about the completeness or representativeness of the data, but these considerations are likely of limited importance since drug laws hold for every resident within a state (apart from occasional municipal legislation regarding marijuana possession) and the predictions regarding bunching follow.

[9] Travova (2019) finds evidence of quantity manipulation by police in Russia, incentivized by their performance evaluation system. The setting is very different, but they find that police officers push offenders above cutoffs, which would go against finding evidence of strategic offender bunching.

[10] Officers have substantial discretion in manipulating speed for traffic stops and the severity of the crime is much lower, typically warranting a fine. In contrast, seized drugs constitute evidence in the eventual establishment of charges and prosecution for potential felony crimes. Manipulation would constitute a crime (tampering with evidence), potentially warranting job loss and legal prosecution. Further, law enforcement agencies typically enforce strict protocols and a chain of custody for weighting and handling evidence, and if an officer wishes to punish an offender more or less than the quantity of drugs dictates, they may possess easier means such as discretion in setting charges.

[11] Namely, very large seizures are likely made before the contraband is processed and distributed for sale, which is a likely step where bunching could occur by packaging the drugs into specific amounts. Similarly, strategic bunching may at least partly be demand rather than supply driven, in which case it would not be identified in Tuttle (2019).

[12] Especially for hard drugs, it seems implausible that police officers would routinely "give breaks" to offenders for excess quantities worth hundreds of dollars and warranting several years in prison.

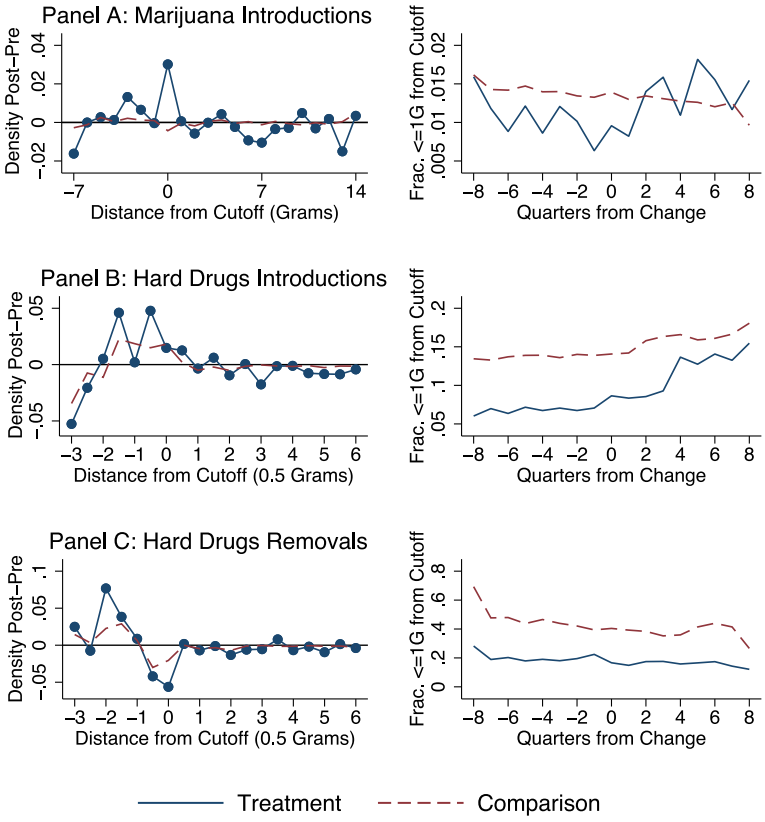

Fig. 3. Bunching and changes in cutoff policies. The left column displays changes in the likelihood that offenders carry given quantities of contraband around cutoffs (normalized at 0) before and after changes in cutoff policies in states who introduced them and comparison states. The right column displays the quarterly fraction of offenders locating within one gram below the cutoff (rather than anywhere else) with cutoff changes normalized at quarter 0, in states who introduced them and comparison states. See Appendix A.1 for states and cutoff changes included in the analysis. Source: NIBRS 1992–2014.

There is bunching at established amounts that are commonly exchanged regardless of cutoff legislation, such as one or two grams, multiples of five grams and fractions of an ounce (28 g). First, this feature of the market is important since cutoffs are also often established at these quantities, requiring a strategy to differentiate between strategic bunching and natural bunching arising from market activity. Second, this feature makes the use of regression discontinuity approaches inadequate (McCrary, 2008), even those adapted for discrete data (Frandsen, 2017), because there may be mass points in density at cutoff quantities even in the absence of strategic bunching. It also highlights another empirical consideration: if few offenders are arrested with amounts other than those standard in the market, then strategic adjustments may be concentrated at a few specific quantities. In that case, cutoffs may increase bunching at the nearest standard quantity below the limit rather than at the limit and dominated regions may be concentrated at a few quantities above the cutoff.

To obtain data on cutoffs and their changes, information on current laws from state criminal codes and past legislative changes from state session laws was merged to the arrest data.[13] To the best of my knowledge, this is a new dataset which allows for the use of novel variation to study criminal behavior. There are a total of 36 cutoff changes arising from 10 legislative reforms in 6 states. See Appendix A.1 for an overview of these changes.[14] I consider reactions to both cutoff introductions and removals, but predictions regarding introductions are clearer and expected adjustments presumably larger. Whenever a cutoff is introduced at a given quantity, we should see increased mass at the limit and missing mass beyond. When a cutoff is removed, fewer offenders may locate at the specific quantity, but failing to adjust can constitute only a second-order loss unless there is a new cutoff below the old one, and expected changes in other quantities around the old cutoff are less clear. Cutoff removals for Marijuana are excluded from the analysis since they mainly reflect decriminalization of these quantities, which presumably affects both offender behavior and reporting.

Some cutoffs apply to possession charges, while others apply to trafficking or selling, and consequences from locating above rather than below vary substantially in their impact on incarceration time and/or fine amounts.[15] Based on state legislation, for cutoffs considered in the analysis, locating above could lead to additional fines averaging $3,000 ($40,000) and additional incarceration averaging 15 months (3.5 years) for marijuana (hard drugs). To obtain evidence on the extent of sentencing discontinuities in practice, Appendix A.2 describes a procedure linking the NIBRS data to the Texas Computerized Criminal History System (CCH)[16] and presents regression discontinuity evidence suggesting increases consistent with the state's criminal laws and corresponding to approximately 40 and 800 additional days of incarceration when locating above the limit of two ounces for marijuana and 1 g for hard drugs. Lastly, this is consistent with Tuttle (2019), which documents sentencing discontinuities in the federal system for locating above cutoffs for crack-cocaine.

Fig. 3 shows changes in the fraction of offenders carrying certain amounts of contraband before and after cutoff changes. The left panels show the difference in the distribution of quantities seized around the cutoff in the two years before and after a cutoff change. Cutoffs are normalized at 0, and the lines essentially represent the difference

[13] I was unable to find information on whether some specific cutoff quantities are established for public health motives such as preventing addiction or overdoses.

[14] There are few changes for marijuana which did not involve legalization, after which amounts below the cutoff are typically no longer reported.

[15] In some cases, locating above the cutoff constitutes prima facie evidence of intent to distribute or trafficking. In most cases, the cutoffs dictate whether the offense is a civil infraction, a misdemeanor, or a felony, and within these classes determine the severity level.

[16] The CCH is a statewide repository of criminal history records including information on arrests, prosecutions and dispositions for cases involving Class B misdemeanors or greater.

between two histograms, with a positive difference indicating that the quantity was relatively more likely to be carried after the introduction. The right panels show time series evidence corresponding to the quarterly fraction of offenders locating within one gram below the cutoff (rather than anywhere else) for two years before and after it became a cutoff. For marijuana, the left side of Panel A shows that quantities are 0.008 or 35% disproportionately more likely to be carried after they become cutoffs, with the increase mostly concentrated at the limit and missing mass within 2 g above the cutoff as well as a quarter and half of an ounce above the cutoff. The right side shows that the marijuana cutoff quantity is disproportionately more likely (approximately 19%) to be carried by offenders after the introduction. Similar evidence for hard drugs is presented in Panel B. There is additional mass within two grams below new cutoffs (approximate 10% increase) and some missing mass mainly concentrated at 2, 3 and 5 g above the cutoff.[17] The time series on the right side of Panel B also show an apparent increase of up to 70% in the likelihood that offenders locate within one gram below a cutoff after its introduction. The time series evidence in Panels A and B suggest that offenders seem to adjust their response to cutoff policies with a delay of 1–3 quarters following cutoff introductions, suggesting a period of adaptation and learning. Lastly, Panel C presents evidence for cutoff removals of hard drugs and shows that quantities appear less likely to be carried after they stop being cutoffs (16% decrease). The graphs also demonstrate the plausibility of the common trends assumption between the two groups of states for the period before changes, although the series for treatment states exhibits more variation due to the smaller sample size, particularly for marijuana.

3. Empirical strategy

The main identification challenge comes from market structure and measurement. Observed quantities could be rounded at various stages of data collection, processing and reporting in a way that may create bunching, and normal market behavior also creates bunching.[18] These concerns motivate the use of variation in cutoff legislation across time and state. If measurement and reporting behavior is stable before and after changes, then looking at differences in bunching isolates strategic bunching.[19]

I use two-year windows before and after each change to minimize the risk that unobserved confounders drive changes in bunching and guarantee no overlap in control and treatment periods for states with multiple changes through time. This leads to the following estimating equation for arrest i and cutoff change c at time t in state s

$$Bunch_{icts} = \beta_0 + \beta_1 T.State_{cs} + \beta_2 Post_{ct} + \beta_3 T.State_{cs} * Post_{ct} + \alpha_s + \eta_t + \varepsilon_{icts} \quad (1)$$

where $Bunch_{icts}$ corresponds to the probability of an offender being arrested with a quantity within one gram below the cutoff, $T.State_{cs}$ is an indicator variable for whether the state implemented change c, $Post_{ct}$ is an indicator variable for the two years before (=0) and after (=1) change c, $T.State_{cs}{}^{*}Post_{ct}$ is an interaction term and α_s and η_t refer to state and time (year and month-of-the-year) fixed effects.

Eq. (1) is estimated by pooling cutoff introductions together and doing the same for cutoff removals. The coefficient of interest, β_3, corresponds to the change in the probability of offenders locating within one gram below the cutoff after new cutoffs are introduced for states which introduced them. Increased bunching at the cutoff may come from both sides of the discontinuity if a nearby cutoff below the new cutoff was removed at the same time, as is the case for several changes. As such, I present two main specifications examining changes in bunching at the cutoff versus anywhere else in the distribution and versus directly above new cutoffs.

In the results section, I present robust standard errors and clustered standard errors at the department originating identifier (ORI) level to allow for arbitrary correlation of errors within arresting agencies. Clustering is complicated by issues of few unbalanced treated clusters, such that standard methods in the literature, including the wild-cluster bootstrap, lead to poor inference.[20] As such, my preferred specification includes robust standard errors for Eq. (1). I also use the synthetic control group approach from Abadie and Gardeazabal (2003) and Abadie et al. (2010), which has been discussed as a solution to issues of few treated units in Cameron and Miller (2015) and acts as a robustness check to the main results by performing inference based on permutation tests. For transparency, control groups were constructed based on matching values of each dependent variable in treatment states for the 8 quarters leading to the change. See Appendix A.3 for weights assigned to the different states.

4. Results

Panel A of Table 1 corresponds to the DID estimates for cutoff introductions. The dependent variable for the first two columns is the fraction of offenders who locate within 1 g below the cutoff (rather than anywhere else), while the dependent variable for columns 3 and 4 corresponds to the fraction of offenders within 1 g below the cutoff rather than above (within one ounce for marijuana and half an ounce for harder drugs).[21] Across all specifications, there is evidence of statistically significant differential increases in bunching at new limits after cutoff introductions in treatment states. The coefficients of interest in columns 1 and 2 correspond to increases of 39% and 16% for marijuana and other drugs respectively, while those in columns 3–4 correspond to increases of 33% and 5%. Differences between the two versions of the dependent variable indicate that some of the increased mass at cutoffs comes from below, consistent with offenders responding to cutoffs being shifted up. This appears more pronounced for hard drugs, as expected since many changes shifted up cutoffs for hard drugs by a few grams. This is inconsistent with police officer manipulation or incapacitation driving the bunching response. Although clustered standard errors are likely too conservative in this setting, as discussed in the previous section, estimates retain statistical significance at the 10% level when clustering at the ORI level, except for the last column. Panel B presents DID evidence for cutoff removals of hard drugs, which indicates that quantities were less likely to be carried after they stopped being cutoffs. The estimated

[17] These patterns of missing mass are consistent with the location of cutoffs. Cutoffs for marijuana are at very common quantities (half an ounce and two ounces) more likely to be carried than other quantities around. Thus, the adjustment is concentrated at the cutoff and missing mass at the next convenient quantities above. For harder drugs, several smaller cutoffs are at 2 or 4 g, which are less likely to be carried than quantities 1–2 g below.

[18] One consideration is whether cutoffs include the limit (e.g. $\leq$ 1G versus $<$ 1G). Most do, but it varies across state and time, which can yield different predictions at the limit. Law enforcement and drug offenders may not make the distinction, especially since it is often unclear even in state criminal codes. In the analysis, I pool cutoff changes regardless of whether they include the limit or not. The main conclusions are unaffected by this choice as shown in Appendix 4.

[19] If law enforcement is more careful when measuring and reporting quantities when they fall near a cutoff, then this could hide evidence of bunching. To the extent that they approximate quantities to common round amounts in the absence of a nearby cutoff, this may accentuate apparent bunching and disappear when a cutoff is introduced, such that using variation from cutoff changes may underestimate the extent of strategic bunching.

[20] See MacKinnon and Webb (2018) for an overview. Depending on the relative size of the few treated clusters, standard adjustments can lead to both severe over or under-rejection. In this case, it is likely to lead to under-rejection, given that most treatment states are comparatively small.

[21] The results are not sensitive to the choice of the region above the cutoff. Appendix A.4 presents results for a region of half an ounce for marijuana and a quarter of an ounce for harder drugs.

Table 1
Differences-in-differences and synthetic control estimates of bunching in response to cutoff changes.

	Fraction at Cutoff		Fraction at Cutoff vs. Above	
	Marijuana	Hard Drugs	Marijuana	Hard Drugs
Panel A: DID Estimates of Cutoff Introductions				
Post	−0.015	−0.003	0.081	-0.030
	(0.000)	(0.001)	(0.005)	(0.003)
	[0.001]	[0.004]	[0.01]	[0.006]
Treat. State	0.006	−0.105	0.211	-0.023
	(0.001)	(0.002)	(0.017)	(0.009)
	[0.001]	[0.033]	[0.07]	[0.053]
Post*Treat. State	0.005	0.020	0.056	0.025
	(0.001)	(0.002)	(0.012)	(0.009)
	[0.003]	[0.012]	[0.029]	[0.025]
Outcome Mean	0.013	0.126	0.170	0.484
N	1,539,799	1,134,685	119,309	296,643
Panel B: DID Estimates of Cutoff Removals				
Post		0.159		0.069
		(0.001)		(0.003)
		[0.007]		[0.005]
Treat. State		−0.074		-0.005
		(0.003)		(0.006)
		[0.027]		[0.031]
Post*Treat. State		−0.012		-0.024
		(0.003)		(0.007)
		[0.018]		[0.016]
Outcome Mean		0.283		0.545
N		767,537		398,277
Year and Month FE	Y	Y	Y	Y
State FE	Y	Y	Y	Y
Panel C: Synthetic Control Estimates of Cutoff Introductions				
Estimate	0.003	0.025	0.057	0.087
RMPSE	0.001	0.000	0.029	0.000
Adjusted P-Value	0.226	0.000	0.241	0.033
Outcome Mean	0.016	0.131	0.175	0.481
Panel D: Synthetic Control Estimates of Cutoff Removals				
Estimate		−0.060		-0.021
RMPSE		0.000		0.012
Adjusted P-Value		0.091		0.563
Outcome Mean		0.260		0.490

Robust standard errors are presented in parentheses and clustered standard errors at the department originating identifier (ORI) level in square brackets. The dependent variable in columns 1 and 2 corresponds to the fraction of offenders locating within one gram below the cutoff. The dependent variable in columns 3 and 4 corresponds to the fraction of offenders locating within one gram below the cutoff rather than within one ounce above for marijuana and half an ounce above for hard drugs. The sample is restricted to two-year windows before and after each cutoff introduction. Adjusted P-Value refers to the proportion of placebos that have a ratio of post-treatment Root Mean Squared Prediction Error (RMSPE) over pre-treatment RMSPE at least as large as the average ratio for the treated units. See Fig. 4 and Appendix A.3 for more information on the synthetic control approach. See Appendix A.1 for states and cutoff changes included in the analysis. Source: NIBRS 2000–2014.

magnitudes are smaller and similar across columns, corresponding to decreases of approximately 4%. The estimates are statistically significant when considering robust standard errors but lose statistical significance when clustering at the ORI level.

Evidence using a synthetic control group approach is presented graphically in Fig. 4. This approach considers a different comparison group chosen to be similar to treatment states based on their pre-period bunching behavior. Panel A corresponds to cutoff introductions for marijuana and Panel B for hard drugs, again presented for both definitions of the dependent variable. Across specifications, there is an apparent increase in bunching in treatment states after cutoff introductions relative to the synthetic control. This approach also helps address concerns from the DID analysis related to potential bunching changes in comparison states, indicated by the statistical significance of the "Post" indicator, since the post-intervention outcome trends are generally flat for the synthetic control. Panel C corresponds to cutoff removals of hard drugs and shows an apparent decrease in bunching at the cutoff, though less pronounced than for introductions.

Panel C of Table 1 provides estimates of the impact of introducing cutoffs from the synthetic control approach, as well as measures of fit and p-values from permutation tests adjusted for the pre-intervention fit between treatment and synthetic control. The magnitudes remain large and positive in all cases, although results regarding marijuana lose statistical significance. The estimates in columns 1–2 correspond to a disproportionate increase of 19% in bunching at the cutoff versus anywhere else for both categories of drugs. In columns 3–4, the estimates correspond to a 32% increase in bunching at the cutoff versus above for marijuana and a 18% increase for harder drugs. Panel D presents similar evidence for cutoff removals, with estimates corresponding to a 24% decrease in column 2 and a 4% decrease in column 4. Only the estimate from column 2 comparing the change at the cutoff versus anywhere else is statistically significant at the 10% level.

Appendix 4 presents robustness checks and heterogeneity analyses. Columns 1 and 2 of Table A2 show that controlling for individual offender characteristics has little impact on the results, while columns 3 and 4 show how the effects vary with these characteristics.[22] Specifi-

[22] Demographic characteristics are not included in the main specification since they are unavailable for a large fraction of the sample.

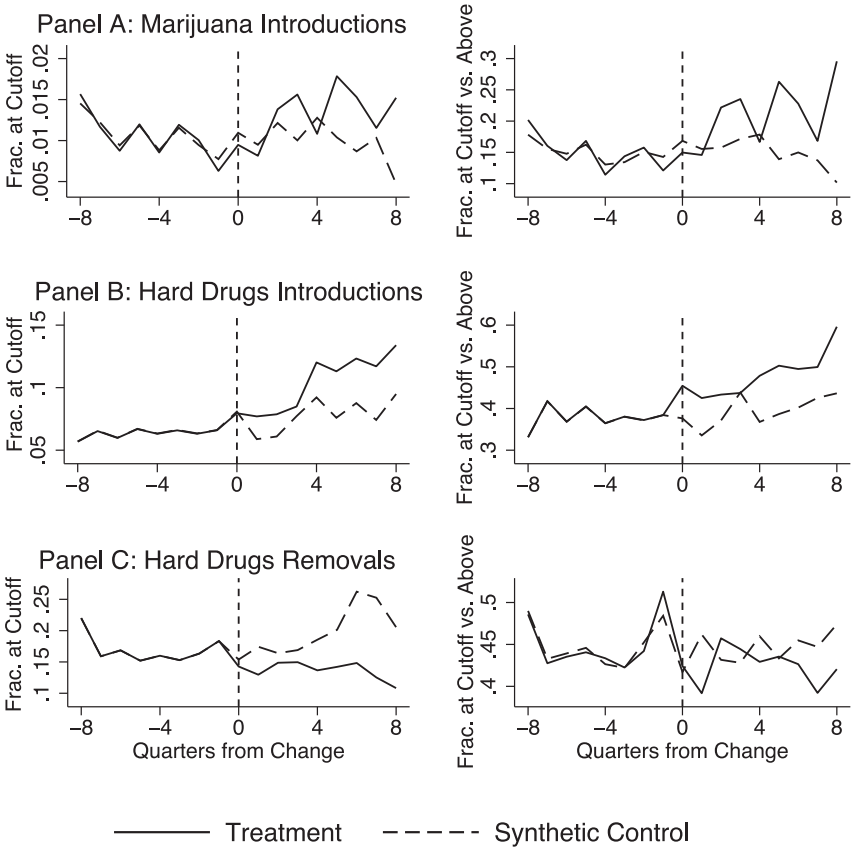

Fig. 4. Synthetic control evidence of bunching and changes in cutoff policies. The left column displays the quarterly fraction of offenders locating within one gram below the cutoff (rather than anywhere else) with cutoff introductions normalized at quarter 0 in states who introduced them and a synthetic control state. The right column displays a similar series contrasting the fraction within one gram below the cutoff to the fraction within one ounce (half of an ounce) above the cutoff for marijuana (hard drugs). The control group was constructed based on matching the value of the dependent variable for each pre-intervention period. See Appendix A.1 for states and cutoff changes included in the analysis and Appendix A.3 for weights assigned to each state. Source: NIBRS 2000–2014.

cally, the increase in bunching for marijuana seems concentrated to non-blacks, larger for women, but not to differ for younger offenders. The increase in bunching for hard drugs is estimated to be similar across demographic groups. These results may appear surprising given evidence on differences in behavior across offender groups, for example younger offenders Lee and McCrary (2017), but interpretation is complicated by the fact that bunching may be at least in part supply-driven. Column 1 of Table A2 shows that the results are robust to considering a narrower one-year time window before and after each change. Column 2 shows that the results do not vary substantively depending on whether cutoffs which include or exclude the limit (e.g. $\leq$ 1G versus $<$ 1G) are considered. Column 3 shows that the increase in bunching seems concentrated exactly at the cutoff for marijuana, while column 4 shows that it appears concentrated at quantities within 0–3 g below the cutoff for hard drugs, in line with evidence from Fig. 3. Columns 5 and 6 show that the increase in bunching for hard drugs seems primarily driven by cutoffs related to possession charges rather than sale or trafficking. This suggests that some of the bunching adjustment may be driven by consumers, but most drug charges overall relate to possession, sellers may only be charged with possession without additional surrounding circumstances, and it is hard to draw any definitive conclusion regarding demand versus supply-driven deterrence given the complicated interplay between demand and supply.[23] In particular, quantities seized likely reflect equilibrium outcomes (quantity packaged to be sold, sold, and carried) rather than quantity demanded or supplied. Column 7 shows that considering a smaller region above cutoffs (half an ounce and a quarter of an ounce for marijuana and hard drugs) has little impact on the results. Column 8 excludes quantities within 5 (2) grams above the cutoff for marijuana (hard drugs) and still shows increased bunching at the cutoff, again suggesting that police officer manipulation is unlikely to explain the observed responses. Similarly, column 9 includes ORI fixed effects to capture the impact of time-invariant arresting agency factors, yielding similar estimates.

A policy evaluation of cutoff legislation is beyond the scope of this paper and would require different data, but one consideration is whether these policies lead to reductions in the total quantity traded and consumed rather than to more, smaller transactions. This also complicates the interpretation of the size of the dominated region, since offenders are not necessarily foregoing these additional quantities altogether. This hypothesis is discussed and tested in Appendix A.5. The evidence is inconclusive, but suggests that the number of transactions below cutoffs increased more (less) than proportionally to the decrease above cutoffs for marijuana (hard drugs). Some of these impacts could also be driven by substitution across drug types caused by relative changes in cutoff quantities, but I am unable to separately identify the two effects given that policy changes often target multiple types of drugs. Relatedly, I find no evidence of changes in the probability that an offender be arrested with a single type of drugs rather than multiple types following cutoff changes.

Lastly, deterrence elasticities, capturing how reactive offenders are to changes in sentencing, vary substantially in the literature, ranging between -0.06 and -0.74 Lee and McCrary (2017). There are some differences and challenges in my setting, making the calculation and interpretation of elasticities more difficult. Perhaps most importantly, offender bunching signals a response to economic incentives, but not necessarily a change in criminality or even the number of offenses, as highlighted above.[24] Nevertheless, bunching elasticities for marijuana correspond to

[23] There are too few cutoff changes for marijuana to investigate this aspect.

[24] Similarly, estimates of the effective change in punishment were obtained for the state of Texas (40 days for Marijuana, 800 days for hard drugs) and can be approximated from state criminal codes, but these may not precisely capture sentencing discontinuities that offenders face in the states that enacted the cutoff changes.

1.77 and 0.22 when using sentencing estimates from the Texas data and from treatment states criminal codes, respectively. For hard drugs, the estimates are of 0.14 and 0.34, respectively. The estimates for marijuana differ vastly between the two denominators, which may be unsurprising given how much enforcement and legislation vary across states and that marijuana-related crimes may be treated as low-severity within an offense category. We may expect a stronger response for hard drugs given larger increases in punishment, but marijuana offenders may be easier to deter if they are less criminally-involved and particularly sensitive to punishment. Overall, while offenders adjust to cutoff policies, these adjustments remain quite small given how large additional penalties from locating above the cutoff can be and the margin of adjustment considered here.

5. Conclusion

Drug laws in many US states give rise to sharp discontinuities in sentencing severity, based on the amount of drugs seized during the arrest. Offenders should respond by bunching to the left of these quantity limits due to the existence of dominated regions immediately beyond cutoffs. This provides a novel setting to study whether offenders are strategic and optimize in response to the sentencing structure. It avoids some important econometric issues common in existing work on deterrence and contributes evidence relevant to larger debates in the economics of crime.

I find robust evidence of strategic adjustments by offenders to these cutoff policies. In the period after a new cutoff is introduced, I find substantial and statistically significant increases in the likelihood that offenders locate at the cutoff rather than anywhere else or above the cutoff specifically. The setting studied here is relatively simple and considers a different margin of adjustment than most existing work. A sizable fraction of offenders, possibly both buyers and sellers, appear not only to know that higher quantities of drugs lead to higher punishment but also exact cutoffs and their changes, providing evidence of sophisticated knowledge of laws.[25] Similarly, the results suggest that these offenders do not ignore the risks of apprehension when committing a crime, without which "profitable deviations" from locating above cutoffs would become large over multiple transactions and may not warrant bunching. This is particularly relevant since evidence summarized in Nagin (2013) highlights that certainty of punishment seems more salient than severity in explaining deterrence.[26] My results suggest that offenders do consider both risks of apprehension and severity of punishment, but that they may only respond when the costs of adjusting to these considerations are relatively low. As such, the extent of these strategic adjustments remains fairly modest given the margin of adjustment considered here and the potential additional penalties faced by offenders who do not bunch.

Lastly, if bunching leads offenders to avoid additional incarceration, it could have important implications for their longer-term outcomes, if longer incarceration affects recidivism and future labor market outcomes. As such, the setting studied here could potentially be used to provide insights on a number of additional related questions.

[25] Ethnography research documents adjustments from drug offenders and particularity sellers to law enforcement policies and that the basic relationship between drug quantity and punishment severity seems to be fairly common knowledge. See Jacobs (2016); Levitt and Venkatesh (2000); Skolnick et al. (1990); VanNostrand and Tewksbury (1999).

[26] Lee and McCrary (2017) propose that this may arise if offenders have high discount rates and punishment is experienced over extended periods of time, such that they put a relatively small weight on punishment severity.

Appendix A

A1. States in the NIBRS and cutoff changes

Table A1
States and years covered by the NIBRS data.

No.	State	Years	Observations
1	Alabama	1992–2014	547
2	Arizona	2004–2014	5655
3	Arkansas	2000–2014	63,077
4	Colorado	1992–2014	164,095
5	Connecticut	1998–2014	85,951
6	Delaware	2001–2014	78,450
7	District of Columbia	2001–2014	1859
8	Georgia	2004–2008	604
9	Idaho	1992–2014	80,559
10	Illinois	1993–2014	20,617
11	Indiana	2003–2014	1093
12	Iowa	1992–2014	111,203
13	Kentucky	1998–2014	66,433
14	Louisiana	2003–2014	19,568
15	Maine	2004–2014	8907
16	Massachusetts	1995–2014	78,056
17	Michigan	1995–2014	461,254
18	Mississippi	2009–2014	5733
19	Missouri	2006–2014	16,264
20	Montana	2005–2014	9203
21	Nebraska	1998–2014	19,919
22	New Hampshire	2002–2014	22,663
23	North Dakota	1992–2014	29,846
24	Ohio	1998–2014	241,167
25	Oregon	2003–2014	47,902
26	Pennsylvania	2013–2014	261
27	Rhode Island	2004–2014	28,438
28	South Caroliana	1992–2014	565,426
29	South Dakota	2001–2014	10,244
30	Tennessee	1997–2014	469,125
31	Texas	1997–2014	177,116
32	Utah	1993–2014	79,782
33	Vermont	1994–2014	25,902
34	Virginia	1994–2014	433,721
35	Washington	2006–2014	34,377
36	West Virginia	1998–2014	67,952
37	Wisconsin	2004–2014	76,189

Source: NIBRS 1992–2014.

Table A2
Summary of cutoff changes.

State	Year	Drug	New Cutoff	Removed Cutoff
Arkansas	2011	Marijuana	0.5OZ	1OZ
		Crack/Cocaine	2G, 10G	1G
Colorado	2010	Marijuana	2OZ	1OZ
		Crack/Cocaine	4G	1G
		Heroin	4G	1G
	2013	Heroin	2G, 7G	4G
Connecticut	2005	Crack		0.5G
Delaware	2002	Heroin	2.5G, 10G	5G
	2003	Crack/Cocaine	10G	5G
	2011	Crack/Cocaine	5G	
	2011	Heroin	1G, 2G	2.5G, 10G
Iowa	2003	Crack	10G	5G
Ohio	2011	Cocaine	20G, 27G	25G
		Crack	20G, 27G	1G, 5G, 10G, 25G

Source: State Session Laws 2000–2014.

A2. Sentencing discontinuities using the CCH

To obtain evidence on sentencing discontinuities in practice, I linked the NIBRS data to the Texas Computerized Criminal History System (CCH), which combines information on both arrests and sentencing (but not seized quantities). Offenders were linked across datasets based on the date of the offense as well as the age, gender and race of the offender, the type of crime and the arresting agency (ORI). Only exact, unique matches were kept, which resulted in a low share of offenses matched (around 20,000 out of 180,000 or 11%). Since a large number of offenses do not result in an incarceration sentence, the number of linked offenses which resulted in an incarceration sentence is 4478.

According to Texas legislation, locating below the two ounce cutoff for marijuana is a Class B misdemeanor punishable by up to 180 days in jail, while locating above constitutes a Class A misdemeanor punishable by up to one year in jail. Locating below the one gram cutoff for hard drugs is a state jail felony punishable by 180–720 days in jail, while locating above constitutes a third degree felony punishable by two to ten years of incarceration.

A Regression Discontinuity Design (RDD) was used to obtain estimates of the sentencing increase from locating above the cutoffs. Fig. 2 presents graphical evidence of the discontinuities using a first order global polynomial with the mimicking variance evenly-spaced method and spacings estimators to select the number of bins. See (Calonico et al., 2015) for more details on the implementation. Also presented in Fig. 2 are RDD estimates computed using local linear regressions with bandwidth selection following Imbens and Kalyanaraman (2012). The graphical evidence shows increases in sentencing from locating above the limit for both types of drugs in the region around cutoffs, with the increase being particularly large for hard drugs as expected. The RDD estimate for marijuana corresponds to a non statistically-significant increase of approximately 40 days, while the estimate for hard drugs corresponds to an increase of around 800 days and is statistically significant at the 1% level.

Overall, this suggests that offenders do face punishment discontinuities if they locate above cutoffs, and these can be much steeper for hard drugs. The estimates are also in line with punishment severity levels outlined in the Texas criminal code, suggesting that using sentencing guidelines from criminal codes can provide a reasonable approximation of the extent of additional punishment faced by offenders.

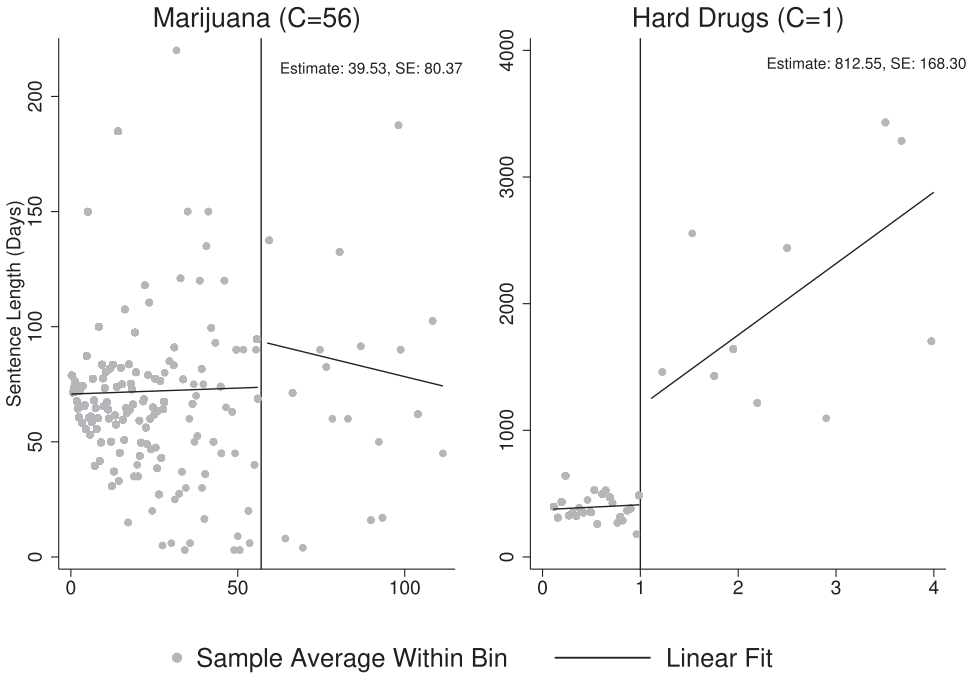

Fig. A1. Texas discontinuities in sentencing. The plots fit a first order global polynomial and use the mimicking variance evenly-spaced method with spacings estimators to select the number of bins (Calonico et al., 2015). The RD estimates are computed using local linear regressions with bandwidth selection following Imbens and Kalyanaraman (2012). Source: Texas CCH and NIBRS 1997–2014.

A3. Synthetic control group weights

Table A3
Synthetic control group weights.

		Fraction at Cutoff			Fraction at Cutoff vs. Above		
		Marijuana	Hard Drugs		Marijuana	Hard Drugs	
			Intro.	Removal		Intro.	Removal
1	Alabama	0.000	0.000	0.000	0.000	0.000	0.000
2	Arizona	0.011	0.001	0.001	0.051	0.000	0.000
3	Arkansas	0.000	0.016	0.001	0.000	0.107	0.063
4	Colorado	0.000	0.004	0.119	0.000	0.089	0.000
5	Connecticut	0.000	0.006	0.008	0.000	0.000	0.000
6	Delaware	0.000	0.374	0.002	0.000	0.000	0.000
7	District of Columbia	0.075	0.020	0.025	0.000	0.000	0.027
8	Georgia	0.000	0.000	0.000	0.000	0.000	0.000
9	Idaho	0.227	0.003	0.002	0.000	0.092	0.000
10	Illinois	0.000	0.003	0.001	0.000	0.000	0.000
11	Indiana	0.000	0.000	0.000	0.000	0.000	0.000
12	Iowa	0.000	0.396	0.251	0.000	0.236	0.475
13	Kentucky	0.000	0.005	0.002	0.000	0.000	0.000
14	Louisiana	0.000	0.003	0.000	0.000	0.000	0.000
15	Maine	0.000	0.004	0.001	0.000	0.000	0.297
16	Massachusetts	0.000	0.003	0.002	0.000	0.000	0.000
17	Michigan	0.000	0.005	0.001	0.000	0.000	0.000
18	Mississippi	0.000	0.007	0.023	0.000	0.000	0.000
19	Missouri	0.000	0.022	0.002	0.026	0.000	0.000
20	Montana	0.000	0.033	0.000	0.000	0.000	0.000
21	Nebraska	0.000	0.003	0.004	0.016	0.000	0.000
22	New Hampshire	0.000	0.004	0.001	0.000	0.000	0.000
23	North Dakota	0.000	0.003	0.001	0.000	0.051	0.000
24	Ohio	0.000	0.002	0.002	0.000	0.000	0.000
25	Oregon	0.000	0.006	0.002	0.000	0.000	0.000
26	Pennsylvania	0.000	0.000	0.000	0.000	0.000	0.000
27	Rhode Island	0.000	0.003	0.003	0.000	0.000	0.000
28	South Caroliana	0.000	0.000	0.001	0.000	0.000	0.000
29	South Dakota	0.000	0.004	0.058	0.060	0.056	0.071
30	Tennessee	0.000	0.004	0.001	0.000	0.000	0.000
31	Texas	0.263	0.007	0.094	0.319	0.000	0.066
32	Utah	0.000	0.007	0.002	0.000	0.243	0.000
33	Vermont	0.000	0.034	0.002	0.000	0.125	0.000
34	Virginia	0.000	0.005	0.002	0.000	0.000	0.000
35	Washington	0.129	0.003	0.384	0.145	0.000	0.000
36	West Virginia	0.000	0.008	0.001	0.000	0.000	0.000
37	Wisconsin	0.294	0.004	0.001	0.384	0.000	0.000

Weights selected based on matching the value of the dependent variable for each pre-intervention quarter. See Fig. 4 and Table 1 for more information on the synthetic control method. Source: NIBRS 1992–2014.

A4. Robustness checks and heterogeneity

Table A4
Regression estimates of bunching in response to cutoff introductions, demographic characteristics and heterogeneity.

	Marijuana	Hard Drugs	Marijuana	Hard Drugs
Post	−0.016	0.000	−0.015	0.012
	(0.000)	(0.001)	(0.001)	(0.002)
Treat. State	0.005	−0.099	0.004	−0.100
	(0.002)	(0.002)	(0.002)	(0.004)
Post*Treat. State	0.006	0.015	0.006	0.009
	(0.001)	(0.003)	(0.002)	(0.005)
Black	−0.002	−0.035	−0.002	−0.019
	(0.000)	(0.001)	(0.000)	(0.001)
Post*Black			0.000	−0.031
			(0.000)	(0.002)
Treat. State*Black			0.007	0.002
			(0.002)	(0.004)
Post*Treat. State*Black			−0.011	−0.001
			(0.003)	(0.006)
Female	−0.002	0.005	−0.002	0.003
	(0.000)	(0.001)	(0.000)	(0.002)
Post*Fem			−0.001	0.006
			(0.001)	(0.002)
Treat. State*Fem			0.000	−0.011
			(0.002)	(0.005)
Post*Treat. State*Fem			0.005	0.002
			(0.003)	(0.008)
Under 25	−0.001	0.013	0.000	0.010
	(0.000)	(0.001)	(0.000)	(0.001)
Post*Under 25			−0.001	0.005
			(0.000)	(0.002)
Treat. State*Under 25			−0.001	0.001
			(0.001)	(0.004)
Post*Treat. State*Under 25			0.002	0.022
			(0.002)	(0.006)
Outcome Mean	0.013	0.128	0.013	0.128
N	1,099,736	699,614	1,099,736	699,614
Month and Year FE	Y	Y	Y	Y
State FE	Y	Y	Y	Y

Robust standard errors are presented in parentheses. The dependent variable corresponds to the fraction of offenders locating within one gram below the cutoff. See Table 1 notes for information on variables, samples and sources.

Table A5
Regression estimates of bunching in response to cutoff introductions, robustness checks and heterogeneity.

	1 Year Window	Inclusive vs Exclusive	Exactly at Cutoff	Within 3G of Cutoff	Possession Cutoffs	Sale/Traff. Cutoffs	Smaller Range Above	Donut Hole	ORI FE
Marijuana									
Post	−0.015		−0.007	−0.033			0.125	−0.016	−0.015
	(0.000)		(0.000)	(0.000)			(0.006)	(0.000)	(0.000)
Treat. State	0.014		0.004	0.011			0.298	0.006	
	(0.002)		(0.001)	(0.002)			(0.023)	(0.001)	
Post*Treat. State	0.004		0.004	0.004			0.067	0.005	0.005
	(0.001)		(0.001)	(0.001)			(0.014)	(0.001)	(0.001)
Outcome Mean	0.013		0.009	0.024			0.213	0.013	0.013
N	766,631		1,539,799	1,539,799			95,275	1,514,100	1,539,615
Hard Drugs									
Post	−0.027	−0.029	0.006	0.040	−0.068	0.050	−0.027	−0.003	0.006
	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)	(0.002)	(0.003)	(0.001)	(0.001)
Treat. State	−0.106	0.050	−0.033	−0.228	−0.174	−0.136	0.008	−0.115	−0.990
	(0.003)	(0.005)	(0.001)	(0.003)	(0.003)	(0.002)	(0.010)	(0.002)	(0.002)
Post*Treat. State	0.015	0.011	−0.002	0.041	0.014	0.001	0.017	0.022	0.016
	(0.003)	(0.006)	(0.001)	(0.003)	(0.002)	(0.002)	(0.010)	(0.002)	(0.003)
Excluding Limit		0.075							
		(0.001)							
Exc.*Post		0.035							
		(0.002)							
Exc.*Treat. State		−0.228							
		(0.005)							
Exc.*Post*Treat. State		0.009							
		(0.006)							
Outcome Mean	0.125	0.126	0.025	0.319	0.130	0.099	0.548	0.134	0.126
N	587,811	1,134,685	1,134,685	1,134,685	805,731	486,032	261,920	1,074,401	1,134,458
Year and Month FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
State FE	Y	Y	Y	Y	Y	Y	Y	Y	N

Robust standard errors are presented in parentheses. The dependent variable corresponds to the fraction of offenders locating within one gram below the cutoff. Column 1 presents estimates for the sample restricted to one year before and after each cutoff introduction. Column 2 presents estimates interacting the change in bunching with whether the new cutoff includes or excludes the limit (e.g. $\leq$ 1G) vs $<$ 1G). Columns 3 and 4 present changes in bunching within one-tenth of a gram and three grams below the cutoff. Column 5 presents estimates of changes in bunching for cutoffs related to possession charges. Column 6 presents estimates of changes in bunching for cutoffs related to sale or trafficking charges. Column 7 presents estimates of changes in bunching within one gram below the cutoff compared to half an ounce (quarter of an ounce) above for marijuana (hard drugs). Column 8 presents estimates of changes in bunching within one gram below the cutoff compared to anywhere else, excluding 5 and 2 g regions directly above the cutoff for marijuana and harder drugs. See Table 1 notes for information on variables, samples and sources.

A5. Number of transactions

Cutoffs could decrease consumption or lead to smaller, more frequent transactions with little change in overall consumption. To see this, consider the problem faced by an offender wishing to acquire or sell quantity Q. The offender chooses the number of transactions t^* and the quantity q_t for transaction $t \in \{1, \ldots, t^*\}$, such that $Q = \sum_{t=1}^{t^*} q_t$. The offender's benefit of transacting Q is given by $B(Q)$. There is a per-transaction cost of c, a continuous cost of p multiplied by q_t, and an additional fixed cost of P for transactions above a cutoff at X. Costs include both financial costs as well as punishment costs weighted by the risk of apprehension. The offender maximizes their net benefit, facing the total cost function $t^*c + \sum_{t=1}^{t^*}[pq_t + \mathbb{1}(q_t > X)P]$. Since increasing the frequency of transactions may increase the probability of arrest, captured through c, offenders face a trade off. Based on the relative magnitudes of the cost components, offenders may locate below, at, or above cutoffs across transactions. They may adjust the volume of transaction, the quantity traded per transaction, and potentially the total quantity traded. Namely, they may be likelier to re-organize transactions with larger excess quantities above the cutoff and give up small excess quantities entirely.

Testing this is challenging, because offenders could adjust transaction amounts in several ways. For example, if the desired transaction is six grams but a cutoff is implemented at five grams, an offender could risk a six gram transaction, engage in a one gram and a five gram transaction or in two two-and-a-half gram transactions. As such, I investigate changes in the number of reports above versus below the cutoff, but a fairly wide interval is necessary to capture potential adjustments of offenders. The NIBRS data is not well suited to testing this type of hypothesis given incomplete and time-varying reporting which may affect the number of transactions.

Evidence is presented in Table A3 using a triple Difference-in-differences specification, comparing changes in the log of the number of quarterly reports after a cutoff introduction across states, time and above versus below the cutoff. Reports within one ounce (half of an ounce) on either side of the cutoff are included in the analysis for marijuana (harder drugs). The coefficients of interest are the triple interactions *Post*Treat.State*BelowCut.* which represent the additional impact of cutoff introductions on the number of reports in treatment states after the introduction and below the cutoff. This can be compared to the coefficients *Post*Treat.State* which represent a similar impact on the number of reports above the cutoff. While the estimated coefficients are not statistically significant, they are of the expected sign with the number of transactions below the cutoff increasing by more than double the amount of the decrease above the cutoff for marijuana but roughly two thirds for harder drugs. This is suggestive that marijuana cutoffs may have led to smaller, more frequent transactions with little change in overall consumption while cutoffs for harder drugs may have led to a decrease in the overall transacted quantity. Ultimately, statistical tests of whether the two estimated coefficients are equal but of opposite sign or whether the change below the cutoff is twice the size of the change above cannot reject the null in all cases.

Table A6
Impact of cutoff introductions on the number of reports.

	Log Number of Quarterly Reports	
	Marijuana	Hard Drugs
Post	−2.446	−0.152
	(0.097)	(0.048)
Treat. State	4.725	0.131
	(0.435)	(0.137)
Below Cutoff	1.720	1.175
	(0.076)	(0.043)
Post*Treat. State	−0.123	−0.135
	(0.374)	(0.176)
Post*Below Cut.	0.014	−0.007
	(0.108)	(0.060)
Treat. State*Below Cut.	−0.440	0.088
	(0.417)	(0.194)
Post*Treat. State*Below Cut.	0.348	0.087
	(0.533)	(0.267)
Outcome Mean	4.266	2.986
N	1625	5039
Month and Year FE	Y	Y
State FE	Y	Y

Robust standard errors presented in parentheses. The dependent variable corresponds to the log of the number of quarterly reports in a state. Reports within one ounce (half of an ounce) on either side of the cutoff are included. The sample size corresponds to the number of state-quarter observations above and below the cutoff in the event windows. See Table 1 notes for information on variables, samples and sources.

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